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E. J. STONE, Esq., M.A., F.R.S., President, in the Chair.

Samuel Jefferson, Esq., 17 Virginia Road, Leeds; and
James Leigh, Esq., Watford House, King's Norton, near Birmingham;

were balloted for, and duly elected Fellows of the Society.

Notes on M. Loewy's Remarks relative to the Wedge-Extinction Method of Stellar Photometry. By Prof. C. Pritchard, F.R.S.

In the *Monthly Notices* for January last, M. Loewy has done me the honour of criticising a method devised by me for the photometric measurement of the relative brightness of the stars, which I somewhat fully described in the *Monthly Notices* of November 1881. He states that he himself and his two associates MM. Paul and Prosper Henry have for several years been engaged in considering various photometric methods devised for the same purpose; and, among other methods, that which I have proposed, and, indeed, to a very considerable extent carried out with what seems to be success. M. Loewy states that the French astronomers abandoned this method. I would here remark that, in a question of this sort, it seems desirable to give numerical instances of a sufficient number of observations, in order that other astronomers may form an independent opinion, and clearly see for themselves the merits or demerits of the question at issue. This I have endeavoured to do in the paper of November last, giving many results as they were obtained direct from the heavens and the photometer. The results there given, though quite satisfactory, were first

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attempts; and could be, and have been, improved by larger experience and additional precautions.

Besides this, the method adopted at Oxford is, I thought and still think, peculiar; and is most probably by no means that which, in its completeness, was tried and is said to have failed at the Paris Observatory. I shall soon explain more at large what I mean as I proceed.

M. Loewy objects to the plan of extinction by a wedge, because (unless I mistranslate or misunderstand his remarks) he found that the reducing of the light by means of the wedge to the extent of seven or eight magnitudes, introduced serious inconveniences.

Now, in my plan of using the wedge, I have uniformly avoided any such process as comparing the light of two stars differing by seven or eight magnitudes, *by means of the wedge alone.* Further, M. Loewy adds that they found it necessary to confine themselves to differences of magnitude not exceeding three magnitudes in the use of the wedge alone. To this I agree *in practice*; but, with the wedge in my possession, I could extend the use of it much further; certainly to six magnitudes without the "introduction of sensible errors."

But what is done, in effect, is this. A star of a magnitude intermediate between those which it is proposed to examine, is selected. In *Lyra*, for instance, κ *Lyræ* was selected about magnitude $4\frac{1}{2}$, and other stars in its neighbourhood, from the first to the eighth magnitude, were examined and compared with it, so that there could be no great or violent differences in the lights compared.

Again, I do not trust to the wedge-extinction process alone; but in such a case as comparing κ *Lyræ* with α *Lyræ*, the brighter star (α) would be observed in a telescope with an aperture of (say) two inches, whereas the fainter star (κ) would be observed with an aperture of 4 inches; thus removing at once an amount of light from the brighter star, equivalent to a magnitude and a half, leaving the wedge to act upon a difference of light amounting to about $2\frac{1}{2}$ magnitudes only, which, in my present wedge, implies the use of less than an inch and a half: and even this amount of wedge interval might be still further reduced, by diminishing the aperture of the telescope to one inch, as is often done in practice at this Observatory. In fact, the telescope used has an aperture of four inches, reducible to three inches, two inches, and one inch, at pleasure. But my experience is not in favour of too violent a reduction of aperture.

Hence it will be observed that my method of photometry is a mixed method of aperture and wedge, each continually testing and verifying the other—a great gain, as I conceive. Thus, I submit, I have successfully met M. Loewy's natural objection to the use of a wedge beyond reasonable limits, however perfect it may be. Probably, however, I did not sufficiently explain this point of practice in my November paper.

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The next objection to the process of wedge-extinction urged by M. Loewy is apparently more formidable. It applies to the material itself of the glass wedge. He states that the Paris astronomers, after a trial of some fifty specimens of neutral-tinted glass, invariably found that they were differently absorbent for different colours. If that were universally the case, and if this difference of absorbing power were decidedly marked, then, I presume, the method of wedge-extinction must be abandoned. But my experience is contrary to that of the French astronomers.

I have submitted the wedge—or, rather, the two wedges—to a scrupulous and exhaustive trial. Their effect on a Sun spectrum I have found to be uniformly absorptive for all the colours, extending from the violet to beyond *B* in the red. A little beyond *B*, noticeable absorption commences. But this is just the part of the spectrum where the light of ordinary stars becomes very feeble, and, consequently, the wedge is trustworthy; excepting, it may be, in the case of a star of a decidedly red colour. But this is, unfortunately, one of the cases where neither the eye nor any photometer that I know can be fully trusted in the comparison of different intensities of light. In a delicate question of this sort, I thought it best to consult one of the highest authorities in this country, Dr. Huggins. He at once referred me to a passage, written by himself, in the *Philosophical Transactions* for 1866, in which he says: "An examination of the neutral-tint glass, with a prism, showed that the absorptive power of the glass, for all refrangibilities in the brighter portion of the spectrum, was very nearly uniform." That eminent physicist was good enough to examine, also, a few other specimens of neutral-tinted glass in his possession, with a substantially similar result; and he adds, in a private communication to me, "In the case of the stars visible to the naked eye, among which there are no very red stars, the wedge would give results quite comparable probably with those of any other method, because any small errors arising from the want of absorptive uniformity would doubtless be much smaller than the unavoidable errors of estimation coming in from other causes."

This cautious expression of opinion, after due and skilful examination on Dr. Huggins' part, entirely coincides with my own experience, after the use of the wedge some thousand times. Thus, I think, M. Loewy's second objection is satisfactorily removed.

M. Loewy then proceeds to explain another method which he had devised, but which he, not unnaturally, doubts if it be new. It is to virtually reduce the aperture of the telescope by means of a moveable pierced diaphragm, placed inside the telescope, and near to the eyepiece. He is quite right in the suspicion that the method is not new. Our well-remembered colleague Mr. Dawes constructed and used a similar apparatus, described in the *Monthly Notices*, vol. xxv. p. 229. But I am

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not aware that Mr. Dawes obtained any considerable results, either by that means, or by another means in which, like myself, he employed variable apertures and wedge combined. The cause, or causes, of failure—for a practical failure it seems to have been—are not difficult now to see. Most probably M. Loewy will avoid them.

I have tried the method of small apertures, but I abandoned it in despair—possibly I was impatient. With a wedge, used unskilfully enough, some five years ago, I was equally impatient and equally unsuccessful. But a patient re-consideration of the whole case has now landed me in what I believe to be a valuable and trustworthy photometric method.

I now proceed to what I regard as an actual test of the reliability and value of the method employed in this Observatory from which may be formed a safe and practical conclusion in regard to the points raised by M. Loewy.

Professor Pickering has devised a photometer of a peculiar construction, depending on double refraction. It resolves itself at last into the comparison or equalisation of lights of two stars placed in a very skilful way in juxtaposition. I need not say that this is the very antithesis of the method I employ. Results obtained by means of the two instruments and the two methods, if they be employed on the same star, must, on comparison, necessarily reveal facts. Now, it so happens, that Professor Pickering has recorded his results of the measurement of the difference of brightness of twenty-six stars of the *Pleiades*. Out of these twenty-six, fifteen have already been observed in Oxford,* as well as some twenty others of this group, not observed at Harvard College. The Oxford observations are the results of three nights' measurement of each star—five readings of the instrument being taken on each night. Finally, the means are taken, and the average deviations of the fifteen separate measures from their general mean, are recorded in the subjoined tables. An inspection of these results will enable astronomers to judge of what has been so far achieved, and what sort of promise the method or methods hold out, for the photometric estimation of relative star-magnitudes.

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* Since the writing of this paper, the photometric measurement of forty-three stars in the *Pleiades* has been completed. Of these, forty have been also carefully measured as to their relative coordinates with the Duplex Micrometer, in order to compare them with Bessel's Heliometer measures. The results, it is expected, will be communicated to the Society in May next.

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TABLE I.

Comparison of the Magnitudes of Fifteen Stars in the Pleiades that have been Photometrically obtained by Observations made at Harvard College and at Oxford.

Bessel's Designation.	Observed Differences of Magnitude from Merope. 1	2	3	Mean difference of Mag.	Average Deviation.	Resulting Oxford Mag. Merope = 4·22.	Resulting Harvard Magnitude.
17 *b* Electra	0·31	0·62	0·41	0·45	0·12	3·77	3·82
16 *g* Celæno	1·01	1·15	1·28	1·15	0·09	5·37	5·23
19 *e* Taygeta	0·35	0·44	0·32	0·37	0·05	4·59	4·44
18 *m*	1·65	1·68	1·53	1·62	0·06	5·84	5·63
21 *k* Asterope	1·52	1·68	1·54	1·58	0·07	5·80	5·71
22 *l*	1·98	2·22	2·15	2·12	0·09	6·34	6·28
20 *c* Maia	0·19	0·50	0·34	0·34	0·10	3·88	3·98
10	2·87	2·78	3·16	2·94	0·15	7·16	6·96
19	2·68	2·64	2·29	2·54	0·16	6·76	6·69
22	2·65	2·59	2·28	2·51	0·15	6·73	6·74
25 *η* Alcyone	1·29	1·34	1·35	1·33	0·02	2·89	3·00
24	2·43	2·12	2·18	2·24	0·12	6·46	6·46
12	2·54	2·27	2·48	2·43	0·11	6·65	6·58
26 *s*	2·34	2·20	2·10	2·21	0·08	6·43	6·36
29	2·54	2·39	2·49	2·47	0·06	6·69	6·56

TABLE II.

Individual and Concluded Measures of the Magnitude of Eighteen Stars in the Pleiades made at Oxford. (Not observed by Prof. Pickering.)

Bessel's Designation.	Diff. from Merope.	Observed Differences of Magnitude from Merope. 1	2	3	Mean difference of Mag.	Average Deviation.	Resulting Oxford Mag. if Merope = 4·22.
1	3·0	2·95	2·99	3·10	3·01	0·06	7·23
4	3·0	3·38	3·36	3·28	3·34	0·04	7·56
6	4·0	5·14	5·00	4·93	5·02	0·08	9·24
7	3·0	3·17	3·36	3·20	3·24	0·08	7·46
8	3·5	3·16	3·11	3·30	3·19	0·07	7·41
9	3·5	3·68	3·67	3·59	3·65	0·04	7·87
13	3·5	3·94	3·70	4·07	3·90	0·14	8·12
24 *p*	2·5	1·66	1·55	1·65	1·62	0·05	5·84
15	3·5	4·30	4·15	4·41	4·29	0·09	8·51
17	3·0	2·36	2·47	2·47	2·43	0·05	6·65
18	3·0	3·78	3·68	3·96	3·81	0·10	8·03
21	3·5	3·74	4·02	3·93	3·90	0·10	8·12
23	3·5	3·19	3·24	3·22	3·22	0·02	7·44
30	3·5	3·55	3·76	3·46	3·59	0·11	7·81
31	3·0	2·41	2·49	2·64	2·51	0·08	6·73
33	3·5	3·06	2·98	2·89	2·98	0·06	7·20
35	4·0	5·29	5·39	5·38	5·35	0·04	9·57
36	4·0	4·50	4·60	4·65	4·58	0·06	8·80

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The only remark necessary to make with regard to the above Tables is that the observations themselves are all differential. The magnitude assigned to *Merope*, which is the Oxford star of comparison, was 4·22, that being the value assigned to it by Professor Pickering. Argelander's value is 4·5. Had I assumed the magnitude of *Merope* as 4·16, instead of 4·22 with Professor Pickering (a course perfectly allowable), then the general agreement of the Oxford with the Harvard magnitudes would have been still more remarkably close.

Professor Pickering, in some valuable remarks which he has contributed to the *Astronomical Register* of February 1882, after kindly expressing the interest which he takes in the Oxford Observations, observes:—"In all forms of the method I have found the observed limit of visibility inconveniently variable, not only on different nights, and for different observers on the same night, but even in successive measures by the same observer at short intervals. The separate determinations included in a single series are sufficiently accordant; but, on returning subsequently to the same object, a new result is often obtained, so that strictly differential measures cannot be made satisfactorily." In reply to this, I have looked carefully through a large mass of observations, made by the same observer on different nights and on the same night, and the result is, that the average deviations of all the several readings from the mean of the whole is sensibly the same as that which is recorded in the *Astronomical Register* as the result of the Harvard Observations. With regard to the fifteen stars in question observed by the two methods, the Harvard mean deviation is 0·13 magnitude; the Oxford, 0·10 magnitude. On this head, too, I have with great interest consulted Professor Pickering's valuable photometric observations, in vol. xi. of the Harvard College Observations, where deviations of a very similar character and amount are exhibited. In fact there seems to me to be a limit to the accuracy to which stellar magnitudes are at present determinable, notwithstanding the skill of the observer or the ingenuity displayed in the construction of his instrument. Nevertheless, that limit is vastly beyond the amount of accuracy which has hitherto been secured by unaided comparisons with the eye.

As to the employment and results of different observers on the same or different nights, I have seen no differences such as in any degree to shake my confidence. I have, in fact, obtained on this head additional confirmation of what I have stated in the addendum to my paper in the *Monthly Notices*, November 1881, p. 11. Nevertheless, it is to be distinctly understood that, for the method of wedge-extinction, it is essential for accuracy that the meteorological circumstances under which any two stars are compared must be sensibly the same. Drifting clouds form the chief difficulty with which the observer has to contend, and I think it probable that it is this circumstance which explains Professor Pickering's remarks on occasional discordant results.

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But meteorological effects on astronomical observations are not confined to photometry.

I have arranged a third Table, from which astronomers may form an opinion of the *advance* which is now being made in the accuracy and consistency of the estimates of the brightness of stars.

Table III.

Comparison of the Photometric Measures of the Magnitude of Fifteen Stars in the Pleiades made at Harvard and Oxford with the estimation of the Magnitude of the same stars made by Bessel, Argelander, and Wolf.

Name of Star.	Difference of Magnitude from Merope,				
	Bessel.	Argelander.	Wolf.	Pickering.	Pritchard.
17 *b* Electra	−0·5	+0·2	−1·0	−0·40	−0·45
16 *g* Celæno	+0·5	+2·0	+0·5	+1·01	+1·15
19 *e* Taygeta	0·0	+0·5	0·0	+0·22	+0·37
18 *m*	+2·0	+1·8	+0·75	+1·41	+1·62
21 κ Asterope	+2·5	+2·5	+1·0	+1·49	+1·58
22 *l*	+2·5	+2·5	+1·5	+2·06	+2·12
20 *c* Maia	0·0	+0·3	−1·0	−0·24	−0·34
10	+3·0	+3·5	+2·25	+2·74	+2·94
19	+3·0	+3·0	+2·75	+2·47	+2·54
22	+3·0	+3·0	+2·75	+2·52	+2·51
25 η Alcyone	−1·5	−1·3	−2·5	−1·22	−1·33
24	+3·0	+2·5	+2·0	+2·24	+2·24
12	+2·5	+3·0	+2·0	+2·36	+2·43
26 *s*	+2·5	+2·5	+2·0	+2·14	+2·21
29	+3·0	+3·3	+2·0	+2·34	+2·47

The minus sign implies that the particular star was brighter than *Merope.*

It is almost unnecessary to point out how closely the photometric measures made at Harvard and Oxford agree, in comparison with the estimations made by the careful and eminent astronomers who have given their attention to the same subject. Nevertheless, it is somewhat curious to remark that the *means* of the estimations made by Bessel, Argelander, and Wolf (widely as they individually differ from each other), agree in the main with the more accurate photometric measures.

Oxford University Observatory:
1882, *March* 9.